irrelevant consideration; and I have never been able to understand the stress laid upon it by acute thinkers. It is because the triangle is *as far as I can perceive* isosceles, that I intuit it to be *as far as I can perceive* equal-angled.

It has, I believe, been already explicitly recognized by certain logicians that a "symbolically" proved conclusion need not give any actual information about "real things." Indeed some go further; but I do not know that any have gone so far as to say that it would not give any information about ideas—although perhaps this may be the logical conclusion.

Cambridge, November 10. E. E. CONSTANCE JONES.

Ice Crystals.

YOUR correspondent, C. M. Irvine (vol. xlvii. p. 31) will find letters on this subject in NATURE, vol. xxxi. pp. 5, 81, 193, 264, 480, and in vol. xxxiii. pp. 461, 486.

Prof. (?) McGee's letter at p. 480, of vol. xxxi., gives a list of communications on the same subject in earlier volumes.

B. WOODD SMITH.

The Late Prof. Tennant on Magic Mirrors.

SEVERAL scientific friends tell me that the late Prof. Tennant, the well-known mineralogist, published some twenty or twenty-five years ago a small pamphlet on Magic Mirrors. Failing to find a copy even in the library of King's College, I invite the readers of NATURE to assist me to discover one.

SILVANUS P. THOMPSON.

City and Guilds Technical College, Finsbury,
November 15.

On a Supposed Law of Metazoan Development.

UNDER the title of "The Relations of *Larvæ* to Adult Forms," I recently read a paper before Section D at the Edinburgh meeting of the British Association. The subject dealt with was of so extensive a nature, and the time available was so limited, that I fear much that was said must have appeared vague and ill-founded, if not entirely incomprehensible. The material of the essay had, indeed, been prepared with the intention of devoting at least an hour to its delivery: as it happened, I found myself under the necessity of cutting out whole passages of my notes whilst speaking.

The few lines of the report in NATURE (vol. xlvi. p. 404), convey a very inadequate idea of what I aimed at proving in the paper, and hence I am tempted to offer a fuller account to the readers of this journal.

The subject of the essay furnishes a problem which must interest every embryologist, even though he should reject the conclusions to which observation and reflection have led me.

In working out the complete paper so many novel and confirmatory points have been met with, so much of importance in the writings of the older embryologists, and more especially in the memoirs of Johannes Müller on the Echinoderm larvæ, has been unearthed, that an extension of the original plan of the work has been rendered necessary.

My conclusions, moreover, are so much in conflict with prevailing doctrines that any haste in producing the full argument would be unpardonable, although a preliminary sketch by way of clearing the ground may be justifiable. On a subsequent occasion an attempt would be made to show how the researches of recent years had, with a few notable exceptions (such as the work of R. S. Bergh, J. Kennel, and N. Kleinenberg), tended away from rather than in the direction of a recognition of the fundamental fact of an alternation of generations as underlying Metazoan development, in that they had been concerned, for example, with unnecessary attempts at homologizing the "mesoderm" and its mode of formation throughout the animal kingdom.

If the facts in support of my case should not be as complete as the published researches of the last thirty years on the ontogeny of very many animals might lead one to anticipate, the circumstance would have an obvious explanation.

With the death of Johannes Müller—a man whose brilliance as an embryologist was only surpassed by his greatness as an anatomist—there closed one chapter, and that one of the finest, in the history of comparative embryology. What influence the publication of "The Origin of Species" had upon the subsequent progress of the science is too well known to need further expatiation here. The pernicious search after pedigrees, initiated by Haeckel, led to an era of activity during which every fact with no apparent bearings on phylogeny was ignored. As a consequence the work of Müller on the Echinoderm larvæ and the essay of Steenstrup on "Alternation of Generations" became more or less mere curiosities in the history of the science. With little exception embryological speculation of the past thirty years has been naught else than a pursuit of will-o'-the-wisps.

It behoves us now to revert to the path along which Johannes Müller laboured.

My own embryological conclusions, like those of contemporaries, have not hitherto been influenced by the *embryological* works of Müller; for it was not until after my paper had been read that a first study of the Echinoderm memoirs convinced me how nearly he had anticipated what follows.

Before passing to the subject, one further remark may be permissible. Owing to lack of time when reading the paper, no opportunity offered itself for pointing out the analogy which obtains between the suggested mode of Metazoan development and the accepted fact of an alternation of generations in the life-histories of all plants above the lowest Thallophytes. Furthermore nothing was said about the mode of formation of the "mesoderm" in certain cases as one or more outgrowths of the endoderm; although the writer was fully alive to the explanation which from his standpoint could be offered. This and other questions of a like character would receive consideration in the complete paper, in which it would be demonstrated that such things and processes need be neither "palingenetic" nor "cœnogenetic," but that the analogy of the formation of imaginal discs in *Insecta*, or in the *Pilidium* of the Nemertine, ought to suffice to account for them. As an instance, the formation of the mesoblastic somites in Amphioxus as evaginations of the endoderm may be only a mode in which certain parts of the adult are in that particular case laid down upon the larva.

And now, after this digression, to return to the question under consideration. Two modes of development have long been distinguished, viz., *larval with metamorphosis* and *fœtal and direct*. Cases are known in which there subsists no homology between the larva and the adult, and even such in which the larva (*Bipinnaria asterigera*) is said to exist apart for a time after it has given rise to the Echinoderm. In many such, moreover, the sole larval organ carried over to the adult is the alimentary tract, all other organs of the larva, such as nervous system, sense organs, locomotor and excretory organs, mouth and anus, &c., being replaced by new formations in the adult. The new organs are thus not homologous with those of the larva; indeed, neither as a whole nor in its parts is the larva the homologue of the adult form; but the latter arises upon the former by a mode of asexual generation.

The birth of the Nemertine on the *Pilidium*, and that of the Echinoderm upon the *Pluteus*, or upon the *Bipinnaria asterigera*, may be cited as examples, and the question may now be asked, What becomes of the larva when (α) food-yolk is more or less abundantly acquired, and (β) when uterine development is initiated? Does the larva really disappear? Anticipating the sequel, it is asserted that the larva never vanishes from the development, but is always present in more or less disguised form. In all cases the adult or imago would appear to arise upon it just as is so obviously the case in the examples previously cited.

In the complete paper the modifications of the process throughout the Metazoa would be considered; in this place generalities alone can be dealt with. If the larva be laded with food-yolk it becomes transformed into a more or less obvious *blastoderm*, upon which the *imago* or mature form takes its origin. Certain of the larval organs—such as those of locomotion—may then disappear, but others, such as the larval excretory and nervous mechanisms (*e.g.*, *Hirudinea*, according to Bergh's researches, *Ichthyopsida* from my own work) would persist. Considerations of space do not permit me to enter fully into details regarding Molluscan development. The published work on this group furnishes one with useful material in support of my case; and the group is an interesting one in connection with this question of the relation of the larva to a blastoderm. In the *Mollusca* one can readily find all gradations from cases in which the adult is gradually substituted for a pelagic larva (*Patella*), through those in which the larva is somewhat burdened with food-yolk (*Buccinum*), to others, finally, in which there is a large yolk-sac and a blastoderm, on which the adult form arises (*Cephalopoda*). Incidentally I may remark that it was the study of some *Buccinum*

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larvæ (*Veligers* with large yolk-sac) three years ago, which first afforded me a key to the solution of the problem of the relation of the larva to food-yolk.

The *Arthropoda* are an important group, for larval forms widely prevail, especially, as is generally admitted, in the lower forms.

The nature of the *Nauplius* is too big a question for discussion here, but Dohrn's conclusion that it is a transformed worm larva (*i.e.* one with *Arthropod* characters) appears to me to represent the truth. Two of the laws governing developmental processes appear to be that larval organs may be transferred to the service of the adult, and (more usually) that adult organs may become larval, or, as thay may be termed, *adaptational larval organs*. Numerous instances of both these could be cited, and the three pairs of appendages in the Nauplius furnish us with a case in point.

In the development of *Mysis* there is an example of the conversion of the *Nauplius* larva, typically represented in the allied *Euphansia*, into a blastoderm upon a yolk-sac.

I venture to attach most weight to the application of the principle to the *Vertebrata*, for it is there that my own work has chiefly lain, and it is undoubtedly the obstacles offered by the phenomena of Vertebrate development which have hitherto prevented the enunciation of the "law of development as an alternation of generations." Larvæ are so commonly encountered among the Invertebrata that the wonder is that no one has inquired why they are so rare, in any guise, in the *Vertebrata*.

In this latter division of the animal kingdom it becomes necessary to approach the problem previously stated as to the fate of the larva when uterine development is initiated.

It may firstly be noted that larval forms, equipped with many adaptational larval organs, are to be encountered in cases with complete segmentation and but little food-yolk, *e.g. Marsipobranchii*, *Ganoidei*, and most *Amphibia*, while a blastoderm on a yolk-sac is characteristic of *Elasmobranchii*, *Teleostii*, and *Sauropsida*, in which a larva, according to the common acceptation, would not be very obvious.

In all these cases, however, larval organs can be proved to exist, and, most important of all, there is a well-marked larval nervous system, which, while not certainly known to persist in any adult form, has been proved to degenerate during the ontogeny of all the oviparous *Ichthyopsida* as yet studied. This apparatus is certainly neither a part of the adult nervous system nor homologous with the latter. For an account of this mechanism the reader may be referred to the papers cited below.[1]

Among other larval structures referred to when reading the paper, the curious degenerating cells on the blastoderm of *Pristiurus*, to which Prof. Van Wijhe once drew my attention, and the knob on the blastoderm of *Torpedo*, as shown in Ziegler's beautiful models of the embryos of this form, and as described by H. E. and F. Ziegler in the *Archiv für Mikroscopische Anatomie*, Bd. xxxix. p. 85, deserve mention.

The yolk-sac viewed as part of the larva would require detailed and extended consideration.

It is gradually broken down by some ferment action on the part of the so-called merocytes,[2] which may possibly represent degenerating cells of the larva. Only towards the close of life in the egg-capsule does the yolk appear to be digested by the alimentary tract of the Elasmobranch. In some reptiles, according to Hans Virchow, the remains of the yolk-sac would appear to be cast off.

In the discussion on my paper, one speaker, a personal friend, hinted that I had been led to look upon the yolk-sac as part of the larva from having followed some stray ends of "larval nerve fibres" on to that structure. I had to confess my regrets that at that time I was unable to lay claim to any such observation; indeed, that having cut my embryos of *Raja batis* without any part of the yolk-sac appended, it had never occurred to me that the fibres described might pass to the yolk-sac. Quite recently it has been seen that at any rate some of the larval "subepiblastic nerves" of the *Anat. Anz.* paper *do undoubtedly make their way to the surface of the yolk sac, lying just beneath its epiblastic covering*. That a further confirmation of my conclusions is to be found in this observation goes without saying.

Some three years ago, when considering the "Inter-relationships of the Ichthyopsida," at a time when this larval question was prominently before me as a fascinating puzzle, I thought that the larva disappeared above the *Ichthyopsida*. I was led to this conclusion by reliance on the accepted belief that larvæ are only met with in aquatic animals, more particularly in marine forms, and by the apparent absence of a larval nervous system above the *Ichthyopsida*. My recent studies and the work of Froriep and Robinson have taught me that this was erroneous.

The larva never disappears, however much it may undergo degeneration.

It may even be doubted if there are not traces of the nervous system of the larva in the ontogeny of the *Amniota*, for there appear to be certain observations of Froriep on reptiles which may admit of interpretation in this sense, and my friend, Dr. Arthur Robinson, tells me that he believes he has found traces of it in certain Mammalian embryos. In mammals, as will be seen, the larva must be regarded as an internal parasite, and like such it would yield up its chief organs. Some remains of its nervous system may, however, persist, as I have proved to be the case in *Mustelus vulgaris*, where the larva is almost as parasitic as in the Mammal. The *Amnion* of the higher Vertebrata is probably also a larval structure with analogies to the organ of the same name in *Insecta*, in the *Pilidium* development, &c., as Kennel had previously insisted. It would appear to me to be a membrane conditioned by the way in which the adult is formed upon the larva.

Another important larval structure is the yolk-sac placenta of *Mustelus lævis* and of many mammals.

In the latter the importance of this organ during a long period of fœtal life has been proved by Hubrecht and Robinson.

The yolk-sac placenta may be explained as due to the fixation of a parasitic larva; indeed, in mammals the larva has become a fixed internal parasite in the uterus, and its mode of life, like that of all internal parasites, leads to great degeneration.

In this connection it may be insisted that it would be contrary to all that we know concerning the effects of the parasitic mode of life to suppose that a form might become a fixed internal parasite, and subsequently becoming freed from its host, attain to a higher grade of organization. Yet this is what we must believe to hold good, if the current views of mammalian development be accepted as correct. From my standpoint, on the contrary, the larva may become a fixed internal parasite, and none the less there may arise upon it a more highly organized and, when fully developed, free-living form, the Mammal.

Witness must be borne to the circumstance that Müller, Kleinenberg, and Kennel have already recognized that in some few divisions of the Invertebrata the mature form always arises upon a larva.

In such groups as the Echinoderms an alternation of generations is now an obvious explanation of the facts, and when so magnificent an investigator as Johannes Müller proved this nearly fifty years ago, one asks, in vain perhaps, why modern embryologists, like Korschelt and Heider in their otherwise admirable "Entwickelungsgeschichte," ignore it. The "recapitulation theory," and the question concerning the nature of the mesoderm have overshadowed the fact and concealed the recognition of an alternation of generations. But the so-called "law of ontogeny" itself is no explanation of the riddles of embryology; at most the recapitulation hypothesis holds for the development of organs, not of organisms.

So far as the facts are available, Metazoan development appears to me to be by means of an alternation of generations, in that from the fertilized egg there arises an organism, the larva, upon which, in one way or another according to the circumstances of each case, a new form, the adult or imago, takes its origin.

In 1855 the veteran zoologist, P. J. Van Beneden, wrote:—"*La génération alternante est un phénomène qu'il faut chercher à faire rentrer dans la loi commune de la reproduction et non pas laisser comme une exception dans la science.*"

In this essay an attempt has been made for the first time to prove that it is "la loi commune de la reproduction" in Metazoa,[1] and in concluding I cannot do better than echo the beautiful aphorism of Gœthe, which in a similar connection has already been commented upon by Steenstrup and Von Baer:—

"*Die Natur geht ihren Gang, und was uns als Ausnahme erscheint ist in der Regel.*"

J. Beard.

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[1] J. Beard: "The Transient Ganglion Cells and their Nerves in *Raja batis*. *Anat. Anz.*, 1892, pp. 191–206; and also, "The Early Development of *Lepidosteus osseus*," Proc. Roy. Soc. London, vol. xlvi. 1889, pp. 115-118.

[2] It is to H. E. Ziegler that we owe most of our knowledge of the way in which these merocytes, in their own degeneration and death, cause the elements of the yolk to become fit for absorption and assimilation.

[1] It is not assumed that all the phenomena classified as "alternations of generations" are alike in their nature.

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